

circular and hyperbolic functions in both differentiation and integration. The gain is for mathematicians; its use to practical men may be doubted, as the numerical calculation of these functions is (at present) best done by the familiar logarithms. In the older treatises the applications were chiefly algebraic and geometric; the author's system is to introduce the student at once to a wide scope of applications in both geometry and physics, including some of the higher branches (*e.g.* central orbits, harmonic vibration, Fourier's and Green's theorems, &c.). It is clear that the account of each must be very brief. In some cases (*e.g.* the article on "Curve-Tracing," Art. 127) it amounts to merely a sketch of procedure and results with scarcely any proof. In an "introductory" work this seems a defect. It is, however, a masterly introduction to the subject, and the wide scope of the applications is well fitted to interest the student.

It remains to notice some defects (in our judgment). About ten pages are devoted to *ordinary* trigonometric relations and tables of mere trigonometric formulæ. This seems too much space (being 4 per cent. of the whole) to such elements. No definition is given of a maximum or minimum, and the treatment of maxima and minima is made to depend wholly on geometry.

On p. 189 it is stated that Taylor's theorem is one "by means of which any function whatever can be expanded"—an obvious slip, corrected lower down (pp. 193, 201). The necessity for the subject-functions, and in many cases also their differential coefficients, being continuous and generally also finite within the limits of any question is not stated. This is, unfortunately, a not uncommon omission in elementary works. ALLAN CUNNINGHAM

*Elementary Algebra.* By Charles Smith, M.A., Fellow and Tutor of Sidney Sussex College, Cambridge. (London: Macmillan and Co., 1886.)

IT is a pleasure to come across an algebra-book which has manifestly not been written in order merely to prepare students to pass an examination. Not that we think Mr. Smith's book unsuitable for this purpose; indeed, with its carefully-worked examples, graduated sets of exercises, and regularly-recurring miscellaneous examination-papers, it compares favourably with the most approved "grinders" books. The real want of the present day is a text-book logically arranged and logically written. Apparently no author cares to risk the chance of the financial ruin of his book by going thoroughly to the root of the evil. A policy of "safety" is the most we can expect. This is Mr. Smith's policy, and although we think he might have made fewer concessions to custom and yet have been safe, we welcome his effort very cordially, trusting that, when his book has gained the success which it well deserves, he will see his way to introduce further improvements. He shows to great advantage as a teacher, his style of exposition being most lucid: the average student ought to find the book easy and pleasant reading. The second set of exercises on the binomial theorem is worth specially noting; in many other mathematical books the sets of exercises proposed to the student might well be, as in this instance, *collections of really useful theorems*.

#### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

#### The Pleomorphism of the Schizophyta

SOME students of natural history are content, when the explanations of phenomena which they have advanced and the

arguments by which they have supported those explanations are appropriated by other observers, to remain silent, trusting to the justice of future generations for the vindication of their claims. So far as my own experience goes, an active observer who should trouble himself to obtain honest treatment from all his contemporaries in regard to the significance of his published writings, might abundantly employ the latter half of his life in struggling with new writers for that just recognition of his efforts in earlier years in advancing the knowledge of this or that subject, which is the one reward desired above all others by most men who have not attained to the heights of philosophic contempt for the regard and sympathy of fellow-labourers. I do not intend to largely employ my leisure in this pursuit, but there is one subject on which I am anxious once for all to establish the significance of my observations and reasonings published twelve years ago in relation to similar views advanced and accepted at this moment.

That subject is what is now spoken of as the pleomorphism of the Schizophyta or Bacteria.

The view that the genera then recently established by Cohn, viz. *Micrococcus*, *Bacterium*, *Bacillus*, *Vibrio*, *Spirillum*, and *Leptothrix*, are form-phases, or variations of growth of a number of "Protean" species of Bacteria, each of which may exhibit, according to undetermined conditions, all or some of these forms, was definitely and precisely formulated by me in my memoir on "A Peach-coloured Bacterium," published in the *Quart. Journ. of Microscop. Science* in 1873. I distinctly recognised the existence of true species of Bacteria or Schizophyta, but I pointed out that these must be characterised, not by the simple form-features used by Cohn, but by the *ensemble* of their morphological and physiological properties as exhibited in their complete life-histories. I illustrated my conception of the Protean or pleomorphic character of Bacterian species by a reference to the similar character of the species of Calcareous Sponges, and I had in my mind also the closely parallel facts established by Carpenter in relation to the endless variety of forms of the Protozoic Foraminifera.

My view was no merely speculative suggestion, but was based upon a careful study of a remarkable peach-coloured Bacterium, which exhibited a wide range of forms, connected by intermediate forms, growing together in the same vessel, and linked to one another most unmistakably by the fact that they all were coloured by a special pigment which I studied with the spectroscope, and to which I gave the name "Bacterio-purpurin." I observed this organism on many different occasions from various localities; I figured and described its various form-phases; I obtained some modifications of form by cultivation, but chiefly depended upon the association of the different forms, the presence of completely transitional forms, and the common bond of the pigment, for the view as to their nature which I put forward. I gave the name *Bacterium rubescens* to this pleomorphic, or, as I termed it, "Protean," species. I gave an account of further observations on this organism in the *Quart. Journ. Mic. Sci.*, 1876, pp. 27-40.

Cohn opposed my view as to the genetic connection of the various forms associated by me under this name, and, contrary to the established laws of nomenclature, substituted a manuscript name in one of Rabenhorst's collections (viz. "*roseo-persicina*"), for the duly-published name applied by me to this organism. He further described some of its form-phases, already figured by me, as *Monas okeni*, *Monas vinosa*, and *Rhabdomonas warmingii*.

On the other hand, two years later, Dr. Warming of Copenhagen (*Vidensk. Meddelelser. naturhist. For. i. Kjöbenhavn*, 1875), after studying the same organism and figuring many of its form-phases, adopted my view as to their nature, and the extension of that view to the Schizophyta generally. He says: "Les bactéries sont douées en réalité d'une plasticité illimitée, et je crois qu'il faudra renoncer au système de M. Cohn." In 1883 Dr. Neelsen, in his "Studien über die blaue Milch" (Cohn's *Beiträge*, vol. iii. p. 241) cites my views and their confirmation by Warming, and rightly contrasts them with the later views of Nägeli and Billroth, and with that of Lister, who conceived that certain Bacteria were developed from a filamentous fungus (*Dematium fuscisporum*). As the result of his investigation of the *Bacterium cyanogenum* of blue milk, Neelsen says: "Viel eher würde für unsern Fall der Ausspruch Lankesters zutreffend erscheinen, von dem Proteus-ähnlichen Organismus, dessen einzelne Erscheinungs-formen eine Serie von Adaptationen vorstellten."

In 1884 Prof. de Bary of Strasburg, in his "Vergleichende

Morphologie der Pilze," p. 511, says, in regard to the question of species among the different forms of Bacteria:—"There exist two views on this subject which are, at any rate in appearance, totally opposed to one another. The first is, as I think erroneously, ascribed to Cohn. . . . Cohn distinguishes merely what we have above spoken of as form-genera and form-species. The other view in its most extreme form amounts to this, that all distinction of species among the Bacteria is denied, and all forms are regarded as modifications of a single species or whatever else it may be called, and these modifications can be transformed by cultivation into one another reciprocally. This view was (if we leave out of consideration older intimations of a similar nature) set up in opposition to Cohn's classification by Lankester in 1873, and by Lister; and in 1874 carried to such a length by Billroth, that he united all the forms of Schizomycetes known to him under one collective species, his *Coccobacteria septica*. It received later a support through the views which Nägeli (1877) expressed in the words, 'I have investigated during the past ten years many thousands of Bacterian forms, and I could not maintain (if I except *Sarcina*) that there was any need for a separation into even two specific forms.' Nägeli, however, adds that he by no means maintains that all forms belong to one single species: it were a bold thing in his opinion to express a definite conclusion in a matter in which morphological observation and physiological experiment leave the investigator so much in the lurch. He expresses himself again in the same sense in 1882. He nevertheless is, when carefully considered, in agreement with Cohn's fundamental conception, since Cohn erected his form-genera and his form-species (the latter based on physiological properties) primarily in order to gain a provisional survey, and irrespective of the question (as he distinctly states) as to whether as thus distinguished they correspond to natural species.

"Nägeli's words above-cited contain a pregnant criticism of the whole controversy, so far as it had then gone. Both parties failed to bring forward (as is especially the case in Billroth's book) the only certain basis for their opinions, namely, the strict observation of the continuity or the non-continuity of the forms or species in question. In the absence of this, our judgment could only remain suspended, more especially since the forms in question are minute, very like to one another, often mixed together, and consequently easily to be mistaken for one another in the absence of quite strict observation. Lankester certainly came somewhat nearer towards establishing a special case of strictly-observed continuity, since the forms of his *Bacterium rubescens* (*Beggiatoa rovo-persicina*) gave evidence of their connection with one another more clearly by their characteristic coloration. Strictly-made morphological and developmental researches are now to hand. They have demonstrated that the forms known as Cocci, Rods, Threads, &c., are phases of growth (*Wuchsformen*)."

Thus writes Prof. de Bary in 1884. To some extent I have reason to thank him for the recognition which he gives to my position in this matter. But I cannot think that he has given a correct statement of my relation to the conclusion which he finally adopts when he associates me with Lister, who derived Bacteria from Fungi, with Billroth, who massed all Bacteria under one collective species, and with Nägeli, who declared that he did not see grounds for distinguishing as many as two.

The view which I put forward in 1873 is *precisely* that which Prof. de Bary now espouses, and I think I may very rightly object to its being confounded with the extreme and exploded theories of other naturalists. As to the "strict morphological and developmental researches" which now have made my doctrine of the pleomorphism of the Schizophytes acceptable to Prof. de Bary, I beg to point out that they do not differ in *character* from my own researches on *Bacterium rubescens*. Prof. de Bary very properly cites the later researches of Cienkowski, Neelsen, Hansen, and Zopf as the chief amongst those which have tended to establish that view as to the forms and species of Schizophyta which I promulgated in 1873. They have done so, *not* by affording us any stricter evidence of actual observation of change of form taking place under the observer's eye, but by multiplying cases similar (in regard to the kind of observation made) to that published by me in 1873, viz. observations of the juxtaposition and structural continuity of different forms, and of the co-existence with extremely divergent forms of abundant intermediate forms.

In relation to the attitude taken up by one of the above-named observers, I have something further to say. Dr. Zopf has made

valuable researches on various Bacteria and on the Mycetozoa, and has published the best systematic account of each of these groups which has appeared. In his quarto memoir (Leipzig, 1882) on the Schizophyta, as well as in the smaller hand-book which he has since produced, Zopf gives a reference to my memoir on "A Peach-coloured Bacterium." He has himself repeated my observations on that organism, but he has entirely abstained from pointing out in the text of his work how far his observations are simply repetitions of those published eleven years previously by me (which they are almost entirely), and he has in the most exact details adopted the view as to the pleomorphism of Bacteria which I then put forward, and on precisely the same grounds, without stating that he had been anticipated by me in this respect.

Not only this, but Zopf actually goes out of his way to ascribe to me a view differing from his own, and one which I have never suggested. Either Zopf is writing about my views without having troubled himself to ascertain what they are, or he is purposely misrepresenting them, when he says ("Morphologie der Spaltpflanzen," 1882, p. v.): "Die Annahme Billroth's und Lankester's dass alle Spaltpilzformen nur Einer einzigen naturhistorischen Art oder Gattung zugehören, lässt sich nicht aufrecht erhalten."

I think Dr. Zopf will find it difficult to bring forward a citation from any writing of mine in which I have hinted, even in the remotest way, that "all the forms of Schizophyta belong to a single natural species." Billroth's declaration on this subject was published a year after my statement of the pleomorphic nature of the numerous natural species of Schizophyta, and never appeared to me to have any foundation in a general botanical experience, but to be the result of the restricted observations of a pathologist.

To remove all possibility of further misapprehension, I may be allowed to quote my own words ("A Peach-coloured Bacterium," *Quart. Journ. Mic. Sci.*, 1873, p. 410):—

"The series of forms which I have found in the growth of *Bacterium rubescens* leads me to suppose that the natural species of these plants are within proper limits 'Protean.' . . . The natural species among the Calcispongiae have been shown by Haeckel not to correspond at all with the series of *forms* distinguished by his predecessors. . . . It seems exceedingly probable that the same manner of regarding the Bacteria will have to be adopted, Cohn's tribes and genera taking the position of an artificial or formal system, whilst the natural species must be based upon some of those more profound characteristics which Cohn has himself indicated to us in his divisions—saprogenous, chromogenous, pathogenous. The indications of natural species do not lie under our hands in the case of the Bacteria, but have yet to be sought out."

I have now, I think, sufficiently pointed out the position of my publication on *Bacterium rubescens* in the history of the modern doctrine of the pleomorphism of the Bacteria. It will accordingly be readily understood that I cannot contentedly see this doctrine referred to, as it was recently in your columns by my friend Dr. Klein, as "Nägeli's theory of the pleomorphism of the Schizophyta," since Nägeli's view was announced four years after my publication, and is not identical with that at present accepted by De Bary, Zopf, and others, which is, in fact, *precisely* that put forward by me in 1873.

Equally objectionable as falsifying the history of knowledge by assigning to one individual the property of another is a statement in your review of Mr. Crookshanks's "Practical Bacteriology" (NATURE, February 18. p. 361). The reviewer quotes and apparently indorses a statement by Mr. Crookshank, whose book I may observe, though useful in many ways, is wanting in accuracy and in references to original sources. The passage to which I allude is as follows:—"Researches," writes our author, "by competent observers have quite recently clearly de-  
"monstrated that several micro-organisms in their life-cycle  
"exhibit successively the shapes characteristic of the orders  
"of Cohn. This had as early as 1873 been observed by  
"Lister in a Bacterium in milk. Lister detected forms of  
"Cocci, Bacteria, Bacilli, and Streptothrix genetically con-  
"nected." Recent observers also have obtained similar  
"results, so that the very foundation of Cohn's classifica-  
"tion has been shaken, and we are left without possessing  
"a sound basis for classification into genera or species."  
In the original work of Mr. Crookshank (p. 110) I find the names of Cienkowski, Neelsen, Zopf, Van Tieghem, and others of my successors in this field cited, but no reference is made to



the memoir published by me in 1873. Lister's observations led him to quite different conclusions, which he has since abandoned. I am sure that those who are at present busy in this country with the study of Bacteria, and who undertake to write hand-books of the subject, can have no desire to do otherwise than give a just statement of the history of knowledge of the organisms of which they treat. Hence it is with no unfriendly feeling that I offer to Mr. Crookshank and other writers similarly engaged the statement contained in this letter.

February 20

E. RAY LANKESTER

### Notes on the Volcanic Phenomena of Central Madagascar

MADAGASCAR is as yet almost a *terra incognita* to the geologist; nothing, so far as I am aware, but notices of the most vague and fragmentary kind ever having appeared in regard to its geological features. Nor indeed may we expect to have other than the most general descriptions until the island is surveyed by thoroughly competent men. In the absence of something more complete, I hope that the following notes on the volcanic phenomena of Central Madagascar may not be unacceptable to your readers, and may prove a contribution, however slight, to our knowledge of the geology of this great island. And first let me mention the volcanic cones, of which there are many scores, probably hundreds, in the part of the island of which we are speaking. These volcanic cones are situated in two localities especially: in Mandridrano, on the western side of Lake Itasy, and in the neighbourhood of Betafo in Vakin' Ankaratra; the former being from 50 to 60 miles west, and the latter from 70 to 80 miles south-west, of Antananarivo, the capital. Both localities are about 130 miles from the sea on the eastern side of the island, and 150 on the western side. It is hardly necessary to say that all these volcanoes are extinct, and that there are none in activity at the present time in any part of Madagascar.<sup>1</sup> On the west side of Itasy the volcanic cones exist in great numbers, and these, therefore, shall be first described.

The extinct volcanoes of this district of Mandridrano extend for a distance of about 20 miles north and south, and perhaps 3 or 4 east and west. They are, for the most part, scoria cones. The cones are thickly studded over the district, in some parts clustering together more thickly than in others. There is no single large volcano to which the others are subsidiary, or upon which they are parasitic. Occasionally there is a series of cones which have evidently been heaped up by the simultaneous ejection of scorix from different vents situated on the same line of fissure, but so that the cones have run one into the other, leaving a ridge, generally curvilinear, at the summit. None of these extinct volcanoes reach the height of 1000 feet. Kasige, which is probably the highest, I found by aneroid to be 863 feet above the plain (5893 feet above the sea). Andranonatoa is perhaps next in height to Kasige. Kasige is a remarkably perfect and fresh-looking volcano, whose sides slope at an angle of about 40°. The scorix on the sides have become sufficiently disintegrated to form a soil on which are found a by no means scanty flora; for among other plants growing here I found an aloe (*A. macroclada*), and clematis (*C. trifida*), two or three Composite herbs (*Senecio cochlearifolius*, *Helichrysium lycopodioides*, *Laggera alata*, &c.), some grasses (*Imperata arundinacea*, &c.), a species of Indigofera, and an orchid. On its top is an unbreached crater, which measures, from the highest point of its rim, 243 feet in depth. It may be mentioned in passing, that on the very summit, in a hollow "cinder," there were found a small piece of money, perhaps of the value of a halfpenny, and a small bead, as also a portion of a ba'ana leaf, with a few pieces of a manioc, and two or three earth-nuts placed upon

it these had been deposited there by some of the heathen inhabitants of the place as a votive offering either to their ancestors or to the Vazimba (the aborigines of Central Madagascar). Continuous with Kasige, and adjoining its south side, though not so high, there is another volcano, Ambohimalala, and dozens of others are to be seen near by.

One thing with regard to these volcanic piles soon strikes the observer, which is, that they are frequently lop-sided, one side of the crater being higher than the other. The higher side varies from north to north-west and west. This is undoubtedly accounted for by the direction of the wind during the eruption, causing the ejected fragments to accumulate on the leeward side of the vent. Now we know that the south-east trades blow during the greater part of the year in Madagascar, hence the unequal development of the sides of the cones. The same thing may be also observed in the volcanic piles in the neighbourhood of Betafo. This phenomenon, as is well known, occurs also in other parts of the world.

A very large number of the cones have breached craters, whence lava has flowed in numerous streams and floods, covering the plains around. These streams and floods consist in every instance, I believe, of black basaltic lava; a sheet of this lava, the mingled streams of which have flowed from Ambohimalala and some other vents, has covered the plain at the foot of Kasige to such an extent as almost to surround the mountain. Similar sheets are to be seen in other parts of the district, but they are so much alike that a description of one will suffice for all. Amboditaimamo (or Ambohitratrimo?) is a small volcano to the north of Lake Itasy, and at the northern confines of the volcanic district. It possesses a breached crater turned towards the east; from this has issued a stream of lava which, following the direction of the lowest level of the ground, has swept through a small valley round the northern end of the mountain, and spread out at its west foot. This sheet of lava, which is horribly rough on the surface, occupies but a small area of some two or three square miles. It has been arrested in its flow in front by the side of the low hills. It is cut through in one part by a stream which, in some places, has worn a channel to the depth of 80 or 90 feet. Its surface, which is slightly cellular, is covered by some hundreds of mammiform hillocks, which must have been formed during the cooling of the liquid mass. The hillocks are mostly from 20 to 30 feet high, and apparently are heaped-up masses of lava, and not hollow blisters. The lava itself is black, heavy, and compact, being porphyritic with somewhat large crystals of augite. As yet it is scarcely decomposed sufficiently to form much of a soil, though grass grows on it abundantly, and a few other plants are to be seen.

A little to the south of Amboditaimamo there is another volcano, known by the name of Andrarivahy. It is situated on the summit of a ridge of hills—astride of it, so to speak—and from its crater there has been an outflow of what must have been very viscid lava, for, though the sides of the volcano and the ridge of hills form an angle of from 30° to 40°, the ejected matter has set or "guttered" on the slope, only a small portion of it having reached the valley below. This ridge of hills, through which the volcanic orifice has been drilled, is composed entirely of gneiss; and indeed it may be here stated that the whole of these volcanoes, as is the case also with those about Betafo, rest upon a platform of gneiss, chiefly garnetiferous.

Throughout the district numerous fragments of basic lava, trachyte, trachytic tuff, and basaltic conglomerate lie scattered about in abundance. The trachyte is of various shades of yellow and gray, and frequently porphyritic with large crystals of sanidine. Pumice, obsidian, and pitchstone do not seem anywhere to be found.

In addition to the numerous scoria-cones, there may be seen here and there in the district some half-dozen or more bell-shaped hummocks of trachyte. They are for the most part composed of a light-coloured compact rock. This rock, having originally had a highly viscid or pasty consistency, has evidently accumulated, and set immediately over the orifice through which it was extruded; such hummocks are Ingolofotsy, Beteheza, Angavo, Ambasy, Isahadimy, Ambohibe, Antsahondra, &c. Ingolofotsy, situated to the north-west of Itasy, is perhaps the most striking in appearance of these trachytic hummocks. It bears some resemblance to a bell or Turkish fez, except that its sides are furrowed with water-channels and its truncated summit is notched in a remarkable manner. Its height above the plain is 665 feet (5258 feet above the sea); the inclination of its sides averages probably 50°. Adjoining Ingolofotsy on the south-

<sup>1</sup> Scrope, in his "Volcanoes," second edition, p. 428, says of Madagascar, "There is some reason to believe in the existence of active volcanic vents in this great island;" and Dr. Daubeny, in the second edition of his "Volcanoes," p. 433, in referring to the islands on the eastern coast of Africa, says: "The principal of these are the great Island of Madagascar, the Isle of Bourbon, and the Mauritius, the first of which has been too little explored to allow of my announcing with certainty anything respecting its physical structure;" and in a note he adds: "Madagascar is stated by Daubuisson to contain volcanoes, on the authority of Ebel (*Bau der Erde*, tome ii. p. 289), who reports that in this island there is a volcano ejecting a stream of water to a sufficient height to be visible 20 leagues out at sea." What remarkable eyesight those from whom Daubuisson heard the story must have had to see an invisible phenomenon so far away! Dr. Daubeny continues: "Sir Roderick Murchison, December 1827, exhibited at the Geological Society some specimens of a volcanic nature, said to have come from this island, but the locality was not mentioned."